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I cannot divest myself of the belief that had Sir John Franklin been aware of the existence of a channel eastward of King William's *Land* (so named until 1854), and sheltered from this impenetrable ice-stream, his ships would safely and speedily have passed through it in 1846, and from thence with comparative ease to Behring Strait.

Having enumerated the different subjects which have engaged the attention of the officers and myself and have employed much of our time, it only remains for me to express a hope that these will be found to be in some measure a justification of any moderate expectations which the President and Council of the Royal Society may have formed at the time of my departure from England in 1857, or at least to afford proof that my desire to be rendered useful in the advancement of science has in no degree abated since then.

I am, Sir, your obedient Servant,

F. L. McCLINTOCK,

To the Secretary of the Royal Society.

Captain R.N.

November 24, 1859.

Major-General SABINE, R.A., Treasurer and V.P., in the
Chair.

In accordance with the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President.—Sir Benjamin Collins Brodie, Bart., D.C.L.

Treasurer.—Major-General Edward Sabine, R.A., D.C.L.

Secretaries.— $\left\{ \begin{array}{l} \text{William Sharpey, M.D.} \\ \text{George Gabriel Stokes, Esq., M.A., D.C.L.} \end{array} \right.$

Foreign Secretary.—William Hallows Miller, Esq., M.A.

Other Members of the Council.—C. Cardale Babington, Esq., M.A.; Rear-Admiral Sir George Back, D.C.L.; Rev. John Barlow, M.A.; Thomas Bell, Esq.; Arthur Cayley, Esq.; William Farr, M.D., D.C.L.; Sir H. Holland, Bart., M.D., D.C.L.; Thomas Henry Huxley, Esq.; Sir Roderick I. Murchison, M.A., D.C.L.; Thomas

Webster, Esq., M.A. ; Rev. William Whewell, D.D. ; Alex. William Williamson, Ph.D. ; Rev. Robert Willis, M.A. ; Sir William Page Wood, D.C.L. ; The Lord Wrottesley, M.A. ; Colonel Philip Yorke.

Thomas Watson, M.D., Frederick Crace Calvert, Esq., John Penn, Esq., and Lieut.-Col. William Yolland, R.E., were admitted into the Society.

The following communications were read :—

- I. “On Recent Theories and Experiments regarding Ice at or near its Melting-point.” By Professor JAMES THOMSON, Queen’s College, Belfast. Communicated by Professor WILLIAM THOMSON, F.R.S. Received September 9, 1859.

My object in the following paper is to discuss briefly the bearings of some of the leading theories of the plasticity and other properties of ice at or near its melting-point, on speculations on the same subject advanced by myself*, and, especially, to offer an explanation of an experiment made by Professor James D. Forbes, which to him and others has seemed to militate against the theory proposed by me, but which, in reality, I believe to be in perfect accordance with that theory.

In the year 1850, Mr. Faraday† invited attention, in a scientific point of view, to the fact that two pieces of moist ice, when placed in contact, will unite together, even when the surrounding temperature is such as to keep them in a thawing state. He attributed this phenomenon to a property which he supposed ice to possess, of tending to solidify water in contact with it, and of tending more strongly to solidify a film or a particle of water when the water has ice in contact with it on both sides than when it has ice on only one side.

In January 1857, Dr. Tyndall, in a paper (by himself and Mr. Huxley) read before the Royal Society and in a lecture delivered at the Royal Institution, adopted this fact as the basis of a theory by which he proposed to explain the viscosity or plasticity of ice, or its capability of undergoing change of form, which was pre-

* Proceedings of Royal Society, May 1857. Also British Association Proceedings, Dublin Meeting, 1857. Also Proceedings of Belfast Literary and Philosophical Society, December 2, 1857.

† Lecture by Mr. Faraday at the Royal Institution, June 7, 1850 ; and Report of that Lecture, Athenæum, 1850, p. 640.

viously known to be the quality in glaciers in virtue of which their motion down their valleys is produced by gravitation. Designating Mr. Faraday's fact under the term "regelation," Dr. Tyndall described the capability of glacier ice to undergo changes of form, as being not true viscosity, but as being the result of vast numbers of successively occurring minute fractures, changes of position of the fractured parts, and regelations of those parts in their new positions. The terms *fracture* and *regelation* then came to be the brief expression of his idea of the plasticity of ice. He appears to have been led to deny the applicability of the term viscosity through the idea that the motion occurs by starts due to the sudden fractures *of parts in themselves not viscous or plastic*. The crackling, he pointed out might, according to circumstances, be made up of separate starts distinctly sensible to the ear and to the touch, or might be so slight and so rapidly repeated as to melt almost into a musical tone. He referred to slight irregular variations in the bending motion of the line marked by a row of pins on a glacier by Prof. Forbes, as being an indication of the absence of any quality that could properly be called viscosity, and of the occurrence of successive fractures and sudden motions in a material not truly viscous or plastic. I can only understand his statements on this subject by supposing that he conceived the material between the cracks to be rigid, or permanent in form, when existing under strains within the limit of its strength, or when strained less than to the point of fracture.

This theory appeared to me to be wrong* ; and I then published,

* While the offering of my own theory as a substitute for Professor Tyndall's views seems the best argument I can adduce against them, still I would point to one special objection to his theory. No matter how fragile, and no matter how much fractured a material may be, yet if its separate fractured parts be not possessed of some property of internal mobility, I cannot see how a succession of fractures is to be perpetuated. A heap of sand or broken glass will either continue standing, or will go down with sudden falls or slips, after which a position of repose will be attained ; and I cannot see how the addition of a principle of reunion could tend to reiterate the fractures after such position of repose has been attained. When these ideas are considered in connexion with the fact that while ice is capable of standing, without immediate fall, as the side of a precipitous crevasse, or of lying without instantaneous slipping on a steeply sloping part of a valley, it can also glide along, with its surface nearly level, or very slightly inclined, I think the improbability of the motion arising from a succession of fractures of a substance having its separate parts devoid of internal mobility will become very apparent. If, on the other hand, any quality of internal mobility be allowed in the fragments between the cracks, a certain degree at least of plasticity or viscosity is assumed,

in a paper communicated to the Royal Society, a theory which had occurred to me mainly in or about the year 1848, or perhaps 1850 ; but which, up till the date of the paper referred to, had only been described to a few friends verbally. That theory of mine may be sketched in outline as follows :—If to a mass of ice at its melting-point, pressures tending to change its form be applied, there will be a continual succession of pressures applied to particular parts—liquefaction occurring in those parts through the lowering of the melting-point by pressure—evolution of the cold by which the so melted portions had been held in the frozen state—dispersion of the water so produced in such directions as will afford relief to the pressure—and recongelation, by the cold previously evolved, of the water on its being relieved from this pressure : and the cycle of operations will then begin again ; for the parts re-congealed, after having been melted, must in their turn, through the yielding of other parts, receive pressures from the applied forces, thereby to be again liquefied and to proceed through successive operations as before.

Professor Tyndall, in papers and lectures subsequent to the publication of this theory, appears to adopt it to some extent, and to endeavour to make its principles cooperate with the views he had previously founded on Mr. Faraday's fact of so called "regelation" *.

Professor James D. Forbes adopts Person's view, that the dissolution of ice is a *gradual*, not a *sudden* process, and so far resembles the tardy liquefaction of fatty bodies or of the metals, which in melting pass through intermediate stages of softness or viscosity. He thinks that ice must essentially be colder than water in contact with in order to explain the observed plasticity or viscosity. That fractures—both large and exceedingly small—both large at rare intervals, and small, momentarily repeated—do, under various circumstances, arise in the plastic yielding of masses of ice, is, of course, an undoubted fact : but it is one which I regard not as the cause, but as a consequence, of the plastic yielding of the mass in the manner supposed in my own theory. It yields by its plasticity in some parts until other parts are overstrained and snap asunder, or perhaps also sometimes slide suddenly past one another.

* I suppose the term regelation has been given by Prof. Tyndall as denoting the second, or mending stage in his theory of "*fracture* and *regelation*." Congelation would seem to me the more proper word to use after fracture, as *regelation* implies previous melting. If my theory of *melting by pressure and freezing again on relief of pressure* be admitted, then the term regelation will come to be quite suitable for a part of the process of the union of the two pieces of ice, though not for the whole, which then ought to be designated as the process of *melting and regelation*.

it; that between the ice and the water there is a film varying in local temperature from side to side, which may be called plastic ice, or viscid water; and that through this film heat must be constantly passing from the water to the ice, and the ice must be wasting away, though the water be what is called *ice-cold*.

There is a manifest difficulty in conceiving the possibility of the state of things here described: and I cannot help thinking that Professor Forbes has been himself in some degree sensible of the difficulty; for in a note of later date by a few months than the paper itself, he amends the expression of his idea by a statement to the effect that if a small quantity of water be enclosed in a cavity in ice, it will undergo a gradual "*regelation*;" that is, that the ice will in this case be gradually increased instead of wasted. In reference to the first case, I would ask,—What becomes of the cold of the ice, supposing there to be no communication with external objects by which heat might be added to or taken from the water and ice jointly considered? Does it go into the water and produce viscosity beyond the limit of the assumed thin film of viscid water at the surface of the ice? Precisely a corresponding question may be put relatively to the second case—that of the large quantity of ice enclosing a small quantity of water in which the reverse process is assumed to occur. Next, let an intermediate case be considered—that of a medium quantity of water in contact with a medium quantity of ice, and in which no heat, nor cold, practically speaking, is communicated to the water or the ice from surrounding objects. This, it is to be observed, is no mere theoretical case, but a perfectly feasible one. The result, evidently, if the previously described theories be correct, ought to be that the mixture of ice and water ought to pass into the state of uniform viscosity. Prof. Forbes's own words distinctly deny the permanence of the water and ice in contact in their two separate states, for he says, "bodies of different temperatures cannot continue so without interaction. The water *must* give off heat to the ice, but it spends it in an insignificant thaw at the surface, *which therefore wastes even though the water be what is called ice-cold.*" Now the conclusion arrived at, namely, that a quantity of viscid water could be produced in the manner described, is, I am satisfied, quite contrary to all experience. No person has ever, by any peculiar application of heat to, or withdrawal of heat from, a quantity of water, rendered it

visibly and tangibly viscid. We even know that water may be cooled much below the ordinary freezing-point and yet remain fluid.

Professor Forbes regards Mr. Faraday's fact of regelation as being one which receives its proper explanation through his theory described above; and, in confirmation of the supposition that ice has a tendency to solidify a film of water in contact with it, and in opposition to the theory given by me, that the regelation is a consequence of the lowering of the melting-point in parts pressed together, he adduces an experiment made by himself, which I admit presents a strong appearance of proving the influence of the ice in solidifying the water, to be not essentially dependent on pressure. This experiment, however, I propose to discuss and explain in the concluding part of the present paper.

Professor Forbes accepts my theory of the plasticity of ice as being so far correct that it points to *some* of the causes which may reasonably be considered, under peculiar circumstances, to impart to a glacier a portion of its plasticity. In the rapid alternations of pressure which take place in the moulding of ice under the Bramah's press, it cannot, he thinks, be doubted that the opinions of myself and my brother Professor Wm. Thomson are verified*.

Mr. Faraday, in his recently published 'Researches in Chemistry and Physics,' still adheres to his original mode of accounting for the phenomenon he had observed, and for which he now adopts the name "regelation;" or, at least, while alluding to the views of Prof. Forbes as possibly being admissible as correct, and to the explanation offered by myself as being probably true in principle, and possibly having a correct bearing on the phenomena of regelation, he considers that the principle originally assumed by himself may after all be the sole cause of the effect. The principle he has in view, he then states as being, when more distinctly expressed, the following:—"In all uniform bodies possessing cohesion, *i. e.* being either in the liquid or the solid state, particles which are surrounded by other particles having the like state with themselves tend to preserve that state, even though subject to variations of temperature, either of elevation or depression, which, if the particles were not so surrounded, would cause

* Forbes 'On the Recent Progress and Present Aspect of the Theory of Glaciers,' p. 12 (being Introduction to a volume of Occasional Papers on the Theory of Glaciers), February 1859.

them instantly to change their condition.” Referring to water in illustration, he says that it may be cooled many degrees below 32° Fahr., and still retain its liquid state; yet that if a piece of the same chemical substance—ice—at a higher temperature be introduced, the cold water freezes and becomes warm. He points out that it is certainly not the change of temperature which causes the freezing; for the ice introduced is warmer than the water; and he says he assumes that it is the difference in the condition of cohesion existing on the different sides of the changing particles which sets them free and causes the change. Exemplifying, in another direction, the principle he is propounding, he refers to the fact that water may be exalted to the temperature of 270° Fahr., at the ordinary pressure of the atmosphere, and yet remain water; but that the introduction of the smallest particle of air or steam will cause it to explode, and at the same time to fall in temperature. He further alludes to numerous other substances—such as acetic acid, sulphur, phosphorus, alcohol, sulphuric acid, ether, and camphine—which manifest like phenomena at their freezing- or boiling-points, to those referred to as occurring with the substance of water, ice, and steam; and he adverts to the observed fact that the contact of extraneous substances with the particles of a fluid usually sets these particles free to change their state, in consequence, he says, of the cohesion between them and the fluid being imperfect; and he instances that glass will permit water to boil in contact with it at 212° Fahr., or by preparation can be made so that water will remain in contact with it at 270° Fahr. without going off into steam; also that glass can be prepared so that water will remain in contact with it at 22° Fahr. without solidification, but that an ordinary piece of glass will set the water off at once to freeze.

He afterwards comes to a point in his reasoning which he admits may be considered as an assumption. It is “that many particles in a given state exert a greater sum of their peculiar cohesive force upon a given particle of the like substance in another state than few can do; and that as a consequence a water particle with ice on one side, and water on the other, is not so apt to become solid as with ice on both sides; also that a particle of ice at the surface of a mass [of ice] in water is not so apt to remain ice as when, being within the mass there is ice on all sides, temperature remaining the same.”

This supposition evidently contains two very distinct hypotheses. The former, which has to do with ice and water present together, I certainly do regard as an assumption, unsupported by any of the phenomena which Mr. Faraday has adduced. The other, which has to do with a particle of ice in the middle of continuous ice, and which assumes that it will not so readily change to water, as another particle of ice in contact with water, I think is to be accepted as probably true. I think the general bearing of all the phenomena he has adduced is to show that the particles of a substance when existing all in one state only, and in continuous contact with one another, or in contact only under special circumstances with other substances, experience *a difficulty of making a beginning of their change of state*, whether from liquid to solid, or from liquid to gaseous, or probably also from solid to liquid : but I do not think anything has been adduced showing a like difficulty as to their undergoing a change of state, when the substance is present in the two states already, or when a beginning of the change has already been made. I think that when water and ice are present together, their freedom to change their state on the slightest addition or abstraction of heat, or the slightest change of pressure, is perfect. I therefore cannot admit the validity of Mr. Faraday's mode of accounting for the phenomena of regelation.

Thus the fact of regelation which Prof. Tyndall has taken as the basis of his theory for explaining the plasticity of ice, does in my opinion as much require explanation as does the plasticity of ice which it is applied to explain. The two observed phenomena, namely the tendency of the separate pieces of ice to unite when in contact, and the plasticity of ice, are indeed, as I believe, cognate results of a common cause. They do not explain one another. They both require explanation ; and that explanation, I consider, is the same for both, and is given by the theory I have myself offered.

I now proceed to discuss the experiment by Prof. Forbes, already referred to as having been adduced in opposition to my theory. He states that mere *contact* without pressure is sufficient to produce the union of two pieces of moist ice * ; and then states, as follows, his experiment by which he supposes that this is proved :—“Two slabs

* “On some Properties of Ice near its Melting-Point,” by Prof. Forbes, Proceedings Royal Soc. Edin., April 1858.

of ice, having their corresponding surfaces ground tolerably flat, were suspended in an inhabited room upon a horizontal glass rod passing through two holes in the plates of ice, so that the plane of the plates was vertical. Contact of the even surfaces was obtained by means of two very weak pieces of watch spring. In an hour and a half the cohesion was so complete, that, when violently broken in pieces, many portions of the plates (which had each a surface of twenty or more square inches) continued united. In fact it appeared as complete as in another experiment where similar surfaces were pressed together by weights." He concludes that the effect of pressure in assisting 'regelation' is principally or solely due to the larger surfaces of contact obtained by the moulding of the surfaces to one another.

I have myself repeated this experiment, and have found the results just described to be fully verified. It was not even necessary to apply the weak pieces of watch-spring, as I found that the pieces of ice, on being merely suspended on the glass rod in contact, would unite themselves strongly in a few hours. Now this fact I explain by the capillary forces of the film of interposed water as follows:—Firstly, the film of water between the two slabs—being held up against gravity by the capillary tension, or contractile force, of its free upper surface, and being distended besides, against the atmospheric pressure, by the same contractile force of its free surface round its whole perimeter, except for a very small space at bottom, from which water trickles away, or is on the point of trickling away—exists under a pressure which, though increasing from above downwards, is everywhere, except at that little space at bottom, less than the atmospheric pressure. Hence the two slabs are urged towards one another by the excess of the external atmospheric pressure above the internal water pressure, and are thus pressed against one another at their places of contact by a force quite notable in its amount. If, for instance, between the two slabs there be a film of water of such size and form as might be represented by a film one inch square, with its upper and lower edges horizontal, and with water trickling from its lower edge, it is easy to show that the slabs will be pressed together by a force equal to the weight of half a cubic inch of water. But so small a film as this would form itself even if the two surfaces of the ice were only very imperfectly fitted to one another. If, again, by better fitting, a film be produced of such size and form as may be represented by a

square film with its sides 4 inches each, the slabs will be urged together by a force equal to the weight of half a cube of water, of which the side is 4 inches; that is, the weight of 32 cubic inches of water or 1·15 pound, which is a very considerable force. Secondly, the film of water existing, as it does, under less than atmospheric pressure, has its freezing-point raised in virtue of the reduced pressure; and it would therefore freeze even at the temperature of the surrounding ice, namely the freezing-point for atmospheric pressure. Much more will it freeze in virtue of the cold given out in the melting by pressure of the ice at the points of contact, where, from the first two causes named above, the two slabs are urged against one another.

The freezing of ice to flannel or to a worsted glove on a warm hand is, I consider, to be attributed partly to capillary attraction acting in similar ways to those just described; but in many of the observed cases of this phenomenon there will also be direct pressures from the hand, or from the weight of the ice, or from other like causes, which will increase the rapidity of the moulding of the ice to the fibres of the wool.

II. "On Spontaneous Evaporation." By BENJAMIN GUY BABINGTON, M.D., F.R.S. &c. Received June 7, 1859. (For Abstract, see p. 127.)

November 30, 1859.

ANNIVERSARY MEETING.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

Colonel Yorke reported, on the part of the Auditors of the Treasurer's Accounts, that the total receipts during the past year, including £3466 3s. 2d. received from the Stevenson bequest, amounted to £7016 0s. 5d.; and the total expenditure, including £2700 invested in the Funds, amounted to £6596 0s. 5d., leaving a balance in the Treasurer's hands amounting to £420.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following lists :—